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ABSTRACT

We investigate the relationship between college openings, college attainment, and health behaviors and outcomes later in life. Though a large prior literature attempts to isolate the causal effect of education on health via instrumental variables (IV), most studies use instruments that affect schooling behavior in childhood or adolescence, i.e. before the college enrollment decision. Our paper examines whether an increase in 2 and 4-year institutions per capita ("college accessibility") in a state contributes to higher college attainment and better health later in life. Using 1980-2015 Census and American Community Survey data, we find consistent evidence that accessibility of public 2-year institutions positively affects schooling attainment and subsequent employment and earnings levels among whites but not among people of color. We then examine how public 2-year accessibility affects twenty health behaviors and outcomes in adulthood by employing restricted-use 1984-2015 National Health Interview Survey data. Only self-reported health is significantly affected by college accessibility among all (white) individuals. Among older men, however, college accessibility has a protective effect on several additional outcomes, including smoking behavior, exercise, the probability of a stroke or heart attack, and mortality.

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Introduction

A large literature examines whether years of formal schooling have a causal effect on health (Galama, Lleras-Muney, and van Kippersluis, 2018). A fundamental issue is that although education and health measures are positively correlated in the data, this need not reflect a causal relationship from education to health. Papers on this topic try to overcome this problem by exploiting variation in schooling that is otherwise unrelated to health; a majority of these use variation in schooling stemming from policies that affect individuals when they are in childhood or adolescence (e.g., compulsory schooling laws or primary/secondary school-building programs), with mixed results (Lleras-Muney, 2022). These scholars have called attention to the need to understand how other margins of schooling affect later health outcomes. This paper seeks to fill this gap by examining how college openings when individuals are young enough to take advantage of them affect health behaviors and outcomes later in adulthood.

Deconstructing the relationship between higher education decisions and health is poignant because the positive education-health gradient is strongest for post-secondary schooling in the case of many health behaviors (Cutler and Lleras-Muney, 2010). Case and Deaton (2021) show that the difference in mortality rates between those with and without a college degree has grown in recent decades. The question of how college access affects college attainment and subsequent outcomes (including health) is also timely from a policy perspective. In the United States, the college wage gap is as high as it has been in 100 years (Goldin and Katz, 2008). At the same time, the percentage of individuals obtaining a college degree has flattened, and the gap in college attainment by family income has grown over time (Page and Scott-Clayton, 2016). These trends, plus the rapidly rising pecuniary cost of college over the past few decades, has renewed policymakers' interest in substantially raising subsidies for higher education, particularly at the 2-year (community) college level and for disadvantaged groups.[1] While recent evidence suggests 2-year tuition subsidies and other programs designed to increase degree completion among 2-year students can raise attainment (Carrell and Sacerdote, 2017; Carruthers and Fox, 2016; Denning, 2017; Evans et al., 2018), there is less evidence on the private and social benefits of such policies (for a recent exception regarding earnings, see Mountjoy, 2022). In particular, how does additional college attainment induced by increased college "access" affect not only individuals' labor market but also health outcomes later in life?

We tackle this question by exploiting the differential build-up of 2 and 4-year institutions per capita across states from 1960-1996, with the lion's share of 2-year college building occurring in the 1960's and early 1970's. Figure 1 shows substantial variation in the number of public 2-year colleges per 18-22 year-old across states and over time, both during and after the build-up period. This is consistent with Kane and Rouse (1999), who show that public 2-year college attendance increased dramatically over this time period from roughly 20% of first-time college attenders to nearly 50%.

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[1] New York, Tennessee, and Oregon have made public 2-year college tuition-free for many families. See http://money.cnn.com/2017/05/11/pf/college/tennessee-free-community-college/index.html (last accessed February 11, 2019).

Our identification strategy is closely related to that of Currie and Moretti (2003; hereafter, "CM"), who exploit differences in the number of colleges at age 17 between cohorts of women observed in the same county and year to examine how schooling affects birth outcomes. By contrast, we examine outcomes for all adults after typical college-going years including earnings, employment, and health outcomes. A difficulty in both CM and our paper is that we do not observe individuals' locations at the time they make college decisions (or even at age 17). Rather, CM only observe county of current residence and assume that was where individuals lived when they made their college choices. In this paper, we examine differences in the number of colleges by state (rather than county) and assign individuals to their state of birth rather than state of residence. Though state of birth also measures "state at age of college decision(s)" with error, it is not subject to bias related to endogenous migration on the part of the respondent.

Using data from the 1980, 1990, and 2000 Census and the 2001-15 American Community Survey (1% PUMS), we find that public 2-year colleges per capita (18–22-year-old) is the only institution type that is significantly associated with educational outcomes. An increase of one public 2-year college per 50,000 individuals in the state (one standard deviation of this variable in the data) when an individual is 17 years old raises years of formal schooling in adulthood by a little more than 0.05 years. This change occurs through a reduction in high-school dropouts and (especially) high-school diploma earners only and an increase in some college attendance as well as 4-year college graduation. On the other hand, public 4-year and private 2- and 4-year institutions per capita have little effect on completed schooling. The effects of 2-year college access are concentrated among whites rather than people of color. Overall, we estimate a financial return to a year of schooling of roughly 9%, which is similar to recent findings in the returns-to-schooling literature (Oreopoulos and Petronijevic, 2013).

In order to examine health behaviors and outcomes, we turn to restricted-use 1984-2015 National Health Interview Survey data (the closest match to our Census/ACS data time period). We examine a large array of different health outcomes (20 in all) in this analysis: self-reported health, smoking, obesity, alcohol consumption, frequency of doctor's visits and hospital stays, flu shot receipt, bed days, activity limitations, sleep hours, depression and mental health, heart attack/stroke, several chronic conditions, and mortality. In our NHIS analysis, we again find that public 2-year accessibility positively affects schooling, particularly among white individuals. However, we find that except for self-reported health, college accessibility is not related to health measures in the overall sample.

On the other hand, there is evidence that older (age 50 and above) men experience improvements in terms of mortality and some other health outcomes. These findings are corroborated by the fact that 1) older men experience the largest increases in educational attainment with respect to 2-year college availability in our data, 2) many papers in the health returns-to-schooling literature have also found larger effects on men than women, likely owing at least in part to biological differences between sexes interacted with external circumstances (Cullen et al., 2016; Goldin and Lleras-Muney, 2019) and underlying differences in lifestyle choices by gender (Cawley and Ruhm, 2011) that may be affected by college attainment, and 3) effects of additional schooling on health outcomes may not appear until older ages, when the cumulative effect of additional health investments over many years begin to be manifest. We discuss these claims further in our Results section below.

Related Literature

Theories linking education and health outcomes are examined by Grossman (1972, 2000) and Galama, Lleras-Muney, and van Kippersluis (2018). After reviewing the literature, Galama, Lleras-Muney, and van Kippersluis (2018) conclude there is meaningful heterogeneity in schooling effects on health outcomes depending on the labor-market returns to additional schooling induced by the policy experiment, the nature of background health care and public health systems, gender and age of study participants, time period under study, and how such additional schooling affects peer groups and marital outcomes. They note a lack of evidence on how interventions other than compulsory schooling reforms—which in many settings likely yield low labor-market returns and may not alter peer groups—affect health outcomes. We contribute to this literature by examining how an increase in college opportunities affect health.

College enrollment and completion may affect health choices and outcomes by raising the productivity of health production (Grossman, 1972), raising lifetime income (Becker, 2007; Cowan, 2011), and changing preferences (for example, via changing peer groups; see Kremer and Levy, 2008; Carrell, Hoekstra, and West, 2011). Though many pathways suggest attending college would improve health, others do not, e.g., if health-harming substances that provide immediate gratification are normal goods. Because health itself is a stock that changes slowly as individuals age, effects of college education on some health measures may not become apparent until individuals are older. Of course, the positive correlation between (college) education and health need not reflect a causal relationship running from more schooling to better health. Other research has found evidence of an effect of early-life health (which is correlated with later-life health) on educational attainment (e.g., Case, Fertig, and Paxson, 2005). Unobserved factors, including cognitive and non-cognitive abilities, risk and time preferences, and individual values can influence both health and education decisions (Fuchs, 1982; Heckman, Stixrud, and Urzua, 2006).

Empirical results reviewed in Galama, Lleras-Muney, and van Kippersluis (2018) and Lleras-Muney (2022) suggest that education affects mortality and smoking, especially for men, in some settings (with little evidence that education affects obesity). Because there are many studies that find null effects, the authors note that findings of protective health effects could be spurious. They add, however, that researchers have generally found more evidence of protective health effects when the returns to schooling are high and/or additional schooling affects the individual's peer group. Thus, our setting of changing college availability provides a strong chance of any positive effects of schooling on health being manifest.

Studies examining the effects of college education on adult health have typically used variation in schooling induced by differences in draft risk surrounding the Vietnam War. They find that the increase in college education due to draft avoidance led to a reduction in mortality (Buckles et al., 2016), smoking (Buckles et al., 2013; De Walque, 2007; Grimard and Parent, 2007), and obesity (Buckles et al., 2013; MacInnis, 2006).[2] Collectively, these papers provide considerable evidence that college education has a positive, causal effect on health, but external validity is limited because the sample is confined to men who were at risk of military induction in the 1960's.[3] By focusing on colleges per capita, we exploit a policy-relevant variable that affects a large swath of young people over several decades and thus may have different effects than does Vietnam War draft avoidance.[4]

CM examine how 2- and 4-year colleges per capita affect birth outcomes, education, and other characteristics for new white mothers, as measured in Vital Statistics Natality files for 1970-1999. Several recent studies use college availability as an instrument for schooling. Kamhofer, Schmitz, and Westphal (2018) use college expansions in Germany to identify the effects of college education on later health outcomes. They find positive average physical health effects but zero mental health effects, along with positive average effects on cognitive ability and wages, of college attainment.

Fletcher and Noghanibehambari (2021) use CM's data on college openings from 1940-57 with combined Census and Social Security death records to examine how college expansions affect old-age mortality. They show that the opening of a nearby 4-year college at age 17 leads to an increase in longevity of 0.13 months and an increase in the likelihood of college attendance by about one percentage point; this corresponds with a treatment-on-the-treated effect of about one additional year of longevity. Compared to our paper, Fletcher and Noghanibehambari (2021) use data from an earlier cohort in which 2-year institutions were much less prevalent. Connolly (2021) focuses on how 2-year college openings from 1920 to 1980 have affected educational attainment, occupation, marital outcomes, and longevity. He finds that community college openings nearby one's place of birth in the post-war era (after 1945) increased the probability of living beyond age 65 by one percentage point.

Our major contribution to the literature on college access and health is that we examine the relationship between access and a host of health behaviors and outcomes for the first time. We do so across a range of adult ages, giving us the chance to see potentially important differences over the life cycle. To the extent that college attainment via increased availability affects mortality, as suggested by recent work, it is important to understand how such changes in mortality come about. For example, is this largely a result of changes in health behaviors (such as smoking or exercise), health care use, or something else? Indeed, Galama, Lleras-Muney, and van Kippersluis (2018) call for more evidence on these questions in their review. Our data and design allow us to tackle these questions.

[2] Buckles et al. (2013) also find that exercise increases and heavy drinking decreases (in some specifications) when years in college increases.
[3] Recent work has also argued that draft risk has a direct effect on health through stress, which challenges the validity of the instrument (see Cawley, de Walque, and Grossman, 2017).
[4] Papers using the related variables of individual proximity to colleges or primary/secondary school openings as instruments for education are abundant in the financial returns-to-schooling literature. For example, see Card (1995), Cameron and Taber (2004), and Heckman, Humphries, and Veramendi (2016). Duflo (2001) uses primary school openings in Indonesia as an instrument to estimate the financial returns to schooling (see also Akresh et al., 2018).

Data

We use data from CM on the number of 4- and 2-year public and private institutions in the U.S. from 1960 to 1996 in our analysis. Lacking information on where subjects lived in young adulthood (or the time they made college enrollment decisions), CM assign colleges (of each type) per 18-22 year-old based on the current county of women in their natality data. They then use alternative datasets (Census and NLSY) in which it is possible to obtain location of residence at age 17 for some individuals to verify that their first-stage results are not driven by higher-educated mothers sorting into locations with more institutions per capita.

In our paper, we aggregate colleges per capita information to the state level. This is because we have access to state of birth, but not county of birth, in the public Census and restricted NHIS data.[5] Assigning college accessibility measures based on state of birth means that we do not have to be concerned that our results are driven by individuals who selectively move into states with greater accessibility prior to college enrollment, or that individuals who already have more education relocate to states with greater accessibility (a concern if such accessibility is correlated with better labor-market prospects for college graduates, for example).

In general, we expect that individuals who are born in states with more institutions per 18-22 year-old when they reach young adulthood (age 17)—which we interchangeably refer to as “college access” or “colleges per capita”—have greater college “access” in the sense that there will be more available slots for potential enrollees, they will live closer to at least one institution (on average), or that tuition will be lower given the greater competition between institutions or because it is correlated with state government institution building priorities. Our measures of access do not distinguish between these possible mechanisms.[6] We do not account for the size of institutions in our measures (aside from distinguishing between 2- and 4-year and public and private) because measuring size by enrollment has a greater chance of conflating college supply with demand. We recognize that the number of institutions itself can be influenced by demand, which we return to in our identification checks below.

Figures 1 and 2 show how the number of public 2- and 4-year colleges per 18-22 year-old, respectively, changed in each state over our sample period. 2-year colleges per capita grew more (in some cases suddenly) than did 4-year colleges in most states, especially during the 1960’s and early 70’s (though the baseline level of 4-year schools per capita is higher overall). Figures 3 and 4 show, respectively, how overall increases in public 2- and 4-year colleges per capita from 1960 to 1996 vary by state. As is noticeable from the figures, the correlation between growth in public 2-year access and 4-year access at the state level is small.

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[5] Data on location at age 17 in the Census data is available for those who are 22 at the time of the survey since there is a question about a respondent’s residence 5 years prior to the interview. However, this is not a suitable sample for our analysis since we want to measure health effects of education throughout adulthood.

[6] In practice, utilizing differences in tuition rates alone does not leave enough variation after including cohort and state fixed effects to perform reliable estimation.

Figure 5 shows the result of regressing state-level first differences in years of schooling attained between the 1960 and 1996 cohorts on the same differences in 2-year colleges per capita. State observations are weighted by their population of 18-22 year-olds in 1960, and larger circles represent more heavily weighted states in the figure. There is a clear positive correlation between these two variables in the figure. It should of course be noted that overall growth in these measures is not what we use to identify effects on schooling and other outcomes in our econometric analysis; rather, it is cohort-by-cohort variation across states and over time that identifies our effects. This is described in the next section.

We first employ the 1980, 1990, and 2000 Census surveys and the 2001-15 American Community Survey (1% PUMS). We include all individuals who are age 22 and above and were 17 years old between 1960 and 1996 (since these are the years for which we have data on their college accessibility variables). This leaves us with over 18 million observations. For computational ease, and because we are focused on state-level policies, we aggregate our data into state by cohort by year of observation cells.

Summary statistics are contained in Table 1. Schooling responses in the Census are assigned to highly disaggregated categories; we create several dummy variables indicating different schooling levels as well as a measure of years of schooling (which we treat as continuous in the analysis). As seen in the table, there are roughly 0.04 (0.03) public 2-year (4-year) institutions per 1,000 18-22 year-olds in a state, on average (compared to 0.06 private 4-year institutions and 0.01 private 2-year institutions per capita). The methods for counting the number of institutions and estimating the population of 18-22 year-olds are discussed in detail in the Data Appendix of Currie and Moretti (2003).

Our National Health Interview Survey (NHIS) sample is from 1984-2015 and contains the restricted-access variable state of birth (for which assignment to colleges per capita at age 17 is necessary, as described earlier). We restrict the age and cohorts of the sample in the same we do for the Census/ACS data. Because state of birth is only available starting in 1984, our sample begins there, rendering the average age of respondents slightly younger than it is in our Census/ACS sample. We examine a host of health-related behaviors, symptoms, and conditions as dependent variables in our analysis. Summary statistics on all variables are included in Table 2. We note that sample sizes differ for each variable, sometimes greatly, because the number of years in which a question was asked varies.

Methodology

We follow Currie and Moretti (2003) in implementing our estimating equations. In particular, our first-stage equation relating educational outcomes to our measures of college access takes the following form:

$$(1)\ Ed_{sct} = \beta_0 + \beta_1 pub2_{sc} + \beta_2 pub4_{sc} + \beta_3 pri2_{sc} + \beta_3 pri4_{sc} + \alpha_s + \gamma_c + \theta_t + X_{sct}\delta + \varepsilon_{sct}.$$

In this equation, Ed_{sct} represents the average education level of cohort *c* born in state *s* observed in year *t*. $pub2_{sc}$ ($pub4_{sc}$) is the number of public 2-year (4-year) institutions per capita that each cohort would have faced at age 17 in their state of birth. $pri2_{sc}$ and $pri4_{sc}$ are defined similarly for 2- and 4-year private institutions. We call these our "college access" measures. α_s represents state of birth dummies, γ_c cohort dummies, and θ_t year dummies. X_{sct} contains additional control variables, including dummies for gender, race, and ethnicity categories as well as age 17 controls for the state per-capita income, k-12 per pupil spending, and unemployment rate.

When we use NHIS data rather than Census data, Equation (1) (and Equations (2) and (3) to follow) is modified to be at the individual level rather than the state-cohort-year level (due to the significantly smaller sample size). Because we examine current health measures as dependent variables in NHIS, we also include state-specific control variables in the year of observation including the unemployment rate, the cigarette tax amount, and the beer tax amount in that analysis.[7]

Our models are identified by cohort differences in college access across states. A recent literature highlights the two-way fixed effect (TWFE) regressions of the type in Equation (1) may not even identify a convex combination of group (e.g., state) specific treatment effects if such effects are heterogeneous over group or time (De Chaisemartin and d'Haultfoeuille, 2020). Because our treatment variable is continuous and non-staggered with all groups experiencing changes from period to period (i.e., no "stayers"), currently available methods that are robust to such heterogeneity do not apply to our setting (De Chaisemartin and d'Haultfoeuille, 2022a). Thus, to ensure that our estimates represent a convex combination of group treatment effects, we must assume that those treatment effects are homogeneous across group and time. However, we also examine the robustness of our results to treatment effect heterogeneity using a modified version of Equation (1) and the method outlined in De Chaisemartin and d'Haultfoeuille (2022b), which we discuss in detail below.

We also examine the reduced-form impact of our college access measures on several additional outcomes in both Census and NHIS:

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[7] Sources for these data are as follows:
State k-12 spending: State Comparisons of Education Statistics: 1969-70 to 1996-97, Table 41. https://nces.ed.gov/pubs98/98018/ (accessed 5/13/19) and Statistics of State School Systems, 1967-68, Table 50. https://catalog.hathitrust.org/Record/000907427 (accessed 5/13/19).
State unemployment rates: Bureau of Labor Statistics: Employment status of the civilian noninstitutional population, annual averages. https://www.bls.gov/lau/rdscnp16.htm (accessed 5/13/19). State unemployment rates, 1957–1975 annual averages, as published in Manpower [or Economic] Report of the President.
State per-capita incomes: Bureau of Economic Analysis, SA1 - Personal Income Summary: Personal Income, Population, Per Capita Personal Income. https://www.bea.gov/iTable/iTable.cfm?reqid=70&step=1&isuri=1&acrdn=6#reqid=70&step=29&isuri=1&7022=21&7023=0&7024=non-industry&7001=421&7090=70 (accessed 5/13/19).
State cigarette taxes: Orzechowski and Walker, The Tax Burden on Tobacco, 2014.
State beer taxes: World Tax Database, http://www.bus.umich.edu/otpr/otpr/OTPRdataV3.asp (accessed 5/24/16) and the Tax Foundation, http://taxfoundation.org/data-taxtopic/925 (accessed 5/24/16).

(2) $Y_{sct} = \beta_0 + \beta_1 pub2_{sc} + \beta_2 pub4_{sc} + \beta_3 pri2_{sc} + \beta_3 pri4_{sc} + \alpha_s + \gamma_c + \theta_t + X_{sct}\delta + \varepsilon_{sct}$.

This allows us to examine the effect of our access measures on labor-market and health outcomes directly. Finally, we can use our access measures as instruments for education to estimate the effect of education on our outcome variables of interest:

(3) $Y_{sct} = \beta_0 + \beta_1 Ed_{sct} + \alpha_s + \gamma_c + \theta_t + X_{sct}\delta + \varepsilon_{sct}$.

Equations (1) and (2) are estimated via OLS, and Equation (3) is estimated via 2-stage least squares. In all equations, standard errors are clustered at the state level to account for intra-state correlation in errors both in a cross section and over time (Bertrand et al., 2004).

Results

1. First-stage results

Table 3 contains first-stage results related to the effects of 2 and 4-year institutions per capita on educational attainment using our full Census sample. The first column indicates that the number of 2-year colleges per capita at age 17 reduces the probability that an individual has less than a high-school diploma. The effect of an increase of one 2-year institution per 1,000 18-22 year-olds in the state is -0.214. This means that an increase of 2-year institutions per 1,000 18-22 year-olds equal to one standard deviation (0.02) lowers the chances of being a high-school dropout by about 0.4 percentage points. For a similar change in 2-year access, the reduction in the probability of only having a high-school diploma is about twice as large. This is accompanied by an increase in the probability of obtaining some college (0.4 percentage points) and graduating from college (0.8 percentage points). Overall, years of schooling increase by about 0.053 years.

Table 3 also shows that none of the other institution types have a significant effect on completed school years. We note that other researchers have found smaller enrollment elasticities with respect to 4-year policy variables (e.g. tuition) than with respect to 2-year ones (examples include Kane, 1995; Cameron and Heckman, 2001). This may be because public 2-year institutions tend to be the cheapest and least exclusive form of U.S. higher education, they attract many “marginal” college goers who are especially sensitive with respect to factors that affect the cost of college. On the other hand, Currie and Moretti (2003) find that both 4 and 2-year colleges per capita in one’s county of residence affect maternal schooling outcomes, and Fletcher and Noghanibehambari (2021) find that mortality effects of college openings are stronger with 4-year institutions (using an earlier set of cohorts than ours). 2-year private institutions are perversely negatively related to years of schooling, though they are by far the least common institution type in the data over our time period.

In Appendix Table 1, we show results from a 2SLS regression (Equation 3) with 2-year public college access serving as the instrument for years of schooling with log of individual income as the dependent variable (as in a typical Mincer regression). We see that one additional year of schooling is associated with a roughly 8.6% increase (statistically significant at the 10% level) in annual earnings, which is typical of IV estimates found in the literature (Card, 2001; Oreopoulos and Petronijevic, 2013).

2. Identification checks

Perhaps the greatest concern regarding the validity of our identification strategy is whether an increase in colleges per capita in a particular state should be viewed as a college supply shock (rather than the product of rising demand). In other words, it may be that unobserved factors correlated with both cohort schooling levels and cohort college availability are in fact driving our results. One way to scrutinize our approach is to examine the effect of college access at different ages simultaneously: if the number of institutions is causing college attainment and not vice versa, then effects should be concentrated at typical college-going ages. Unfortunately, a high degree of serial correlation in number of colleges per capita over time makes it difficult to separately identify effects of college access at different ages.

To deal with this issue, we run our models using cohort differences in our dependent and independent variables rather than levels. To do this, we aggregate the data to the state-cohort level and take 3-cohort differences in years of schooling (the dependent variable) as well as each of our independent variables. The difference in schooling years is then regressed on each of the independent variable differences along with state and cohort fixed effects (in case such effects may not be totally time-invariant). In addition to the 3-cohort difference in public 2-year colleges at age 17, we include 3-cohort differences at ages 7, 12, 22, and 27 (5 and 10 years before and after age 17).[8]

Results of this exercise are shown in Figure 6, which includes point estimates and 95% confidence intervals for each 3-cohort difference in public 2-year access at each age. The only coefficient that is positive and statistically different from zero at the 5% level is the one at age 17. On the other hand, it is again difficult to statistically distinguish effects at age 17 from effects at other ages (note the especially large confidence intervals for later ages). We view the results from this exercise as providing modest support for our identification strategy.

Another way to indirectly assess the exogeneity of public 2-year colleges per capita is to examine its correlation with other state-level factors that potentially affect the educational attainment of cohorts. If 2-year college access were associated with observed factors affecting educational attainment, it could indicate a correlation with unobserved drivers as well (Altonji, Elder, and Taber, 2005; Oster, 2019). This is shown in Table 4. Each column represents results from regressing a different state characteristic as the dependent variable on our college access measures and other controls. As seen in the table, coefficients on 2-year public college access are small and statistically unrelated to age 17 measures of state k-12 spending, income per capita, the unemployment rate, and colleges per capita of all other types (once controls are

[8] In this analysis, we only include public 2-year institutions in the regressions, though results with the other institution types in the regressions are similar (available upon request).

included). This analysis again provides some evidence that treating 2-year public colleges per capita as exogenous is acceptable (see Pei, Pischke, and Schwandt, 2019).

Lastly, we examine the susceptibility of our results to bias that can arise due to heterogeneity in treatment effects as discussed earlier. Though we cannot apply existing robust methods to our data as it stands, we can examine how a discrete treatment related to 2-year colleges per capita (e.g., having at least 0.05 institutions per 1,000 18-22 year-olds) affects some of our outcomes. If 2-year college access is defined via this discrete treatment (which is admittedly arbitrary), we can apply methods from De Chaisemartin and d'Haultfoeuille (2022b), which allow for a non-staggered treatment and dynamic effects. This has the advantage of allowing us to examine whether there are any pre-treatment trends in cohorts that experience a change in treatment relative to those that do not.

De Chaisemartin and d'Haultfoeuille (2022b) define a treatment “event” as occurring the first time a group is treated (in non-staggered designs, the group’s treatment status, as it is usually defined, can change after this point). They then compare the time path of outcomes for a group whose treatment status changes for the first time at a certain point to those groups who remain untreated over the entire time path. They then aggregate these effects over groups and normalize by the amount of time groups are treated to get an average effect of one unit of treatment over the entire post-treatment horizon.

We define treatment as having at least 0.05 2-year public institutions per 1,000 18-22 year-olds in a state. This is roughly equal to the median of the 2-year college per capita distribution. We include the same set of control variables (including per-capita colleges of other types) to execute this estimator as we do in our TWFE regressions. Inference is done via a bootstrap with 200 replications clustered at the state level.

The results, with years of schooling as the dependent variable, are contained in the event-study graph in Figure 7. We allow for 6 pre-treatment “periods” (cohorts) and 12 post-treatment ones given that the majority of treatment status changes occur in the first half of our panel. As seen in the figure, there is little evidence that states that experienced treatment were diverging from untreated states prior to treatment. After treatment, cohorts in treated states steadily build their years of schooling advantage related to those in untreated states before the effects level off. None of these individual effects are significant at the 5% level, however. The average effect of treatment over the entire post-treatment horizon is 0.049 (s.e. of 0.034), which is very similar to the coefficient on this treatment variable in a TWFE regression like Equation (1) with the discrete treatment replacing 2-year institutions per capita (0.049, s.e. of 0.012).[9]

Though this analysis is limited because of the need to discretize our treatment variable, it once again lends support to the hypothesis that 2-year public institutions per capita in a state

[9] We also tried using log individual income in this analysis. We focused on the sample of 31–60-year-old whites for whom our baseline estimates are strongest. The De Chaisemartin and d'Haultfoeuille (2022b) estimator yielded an average effect of 0.18 (s.e. of 0.016) while the TWFE estimator yielded an effect of 0.010 (s.e. of 0.004).

contribute positively to educational attainment of cohorts who are young enough to take advantage of them by the time they are of a typical college age. Because with a discretized treatment we find similar results—at least in terms of magnitudes—between average post-treatment effects that account for heterogeneity and standard two-way fixed effects estimates that do not, this assuages concerns that our more general estimates below suffer from significant heterogeneity bias.

3. Heterogeneity

The final three columns of Table 3 show how 2-year college access (and access at other institution types) is related to individual income, family income, and the probability of current employment for the full sample. Though effects are positive, none are significant at conventional levels. However, this masks significant heterogeneity by race. Table 5 shows results from regressions run on whites and people of color (non-whites, of which about 70 percent are black, and the remainder are another race). Whites experience substantially larger effects of 2-year colleges per capita on schooling and adult income and employment measures than do people of color. An increase of 0.02 2-year institutions per capita raises white individual income by about 1.3%, family income by about 1%, and the probability of employment by about 0.5%. In Appendix Table 2, we show that effects for white males and females are very similar to each other for both years of schooling and all labor-market outcomes analyzed.

The reason(s) why 2-year college access fails to affect the educational outcomes of people of color over this time period are unclear. This finding is consistent with Currie and Moretti (2003), who focus on white mothers because of a lack of an association between college access and schooling among minority individuals. However, more recent interventions to promote college attainment suggest that treatment effects are no different for white and non-white students or perhaps even larger for the latter group (Carrell and Sacerdote, 2017; Denning, 2017; Evans et al., 2018).

Table 6 examines how 2-year college access affects the schooling and labor-market outcomes of whites across the age distribution. For this exercise, we split the sample into the following categories (age 30 and under, 31-40, 41-50, 51-60, and 61 and above).[10] As seen in the table, the effect of 2-year college access on schooling is fairly similar across age. However, income and employment effects vary largely over the age distribution in a way that we would expect: effects are hump-shaped over the age range. In particular, we see strong effects on individual and family income as well as employment for adults in their prime working years: 31-50 and to a lesser extent 51-60 (see Bhuller, Mogstad, and Salvanes, 2017). Effects for younger individuals (who are new to their careers and may not yet have overtaken peers with less education but more experience) and older individuals (who are more likely to be retired) are smaller and generally insignificant.

4. Health outcomes

[10] The oldest cohort in our data was 72 as of the last survey used (2015).

We now turn our attention to analyzing health outcomes in the NHIS. We first attempt to replicate the results from ACS on years of schooling in NHIS, which also asks individuals about their schooling attainment. These results are contained in Table 7. Coefficients on 2-year public colleges per capita are remarkably similar for all individuals as well as white individuals specifically compared to those from the ACS analysis. For people of color, the effect is again statistically insignificant, so we focus our main analysis of health behaviors on whites with effects on people of color shown in Appendix Table 3.[11]

We employ 20 different measures of health behavior and outcomes in our analysis (details on each of these measures are provided in Appendix Table 4). In terms of outcomes, we focus on 1) whether an individual reports their current health as excellent or very good (as opposed to good, fair, or poor), 2) whether an individual is clinically obese (BMI>30), 3) whether the individual spent more than 7 days in bed due to illness or injury in the previous year, 4) whether the individual has activity limitations due to any chronic conditions, 5) whether the individual was hospitalized in the previous year, 6) whether the individual had major or manic depression in the previous year, 7) the individual's Kessler (K6) score, a widely used measure of mental distress ranging from 0 to 24 (with a higher number indicating more distress) based on frequency of depressed or anxious feelings over the past 30 days, 8) whether the individual had lower-back pain in the past 3 months, 9) whether the individual has ever been diagnosed with cancer, 10) whether the individual has ever been diagnosed with a heart attack or stroke, 11) whether the individual has ever been diagnosed with diabetes, hypertension, or coronary heart disease, 12) whether the individual has been diagnosed with emphysema or chronic bronchitis, 13) whether the individual has been diagnosed with a chronic heart condition (other than coronary heart disease), ulcer, weak or failing kidneys, or a chronic liver condition, and 14) whether the individual is deceased.

In terms of health behaviors, we examine 1) an individual's usual hours of sleep per day, 2) whether the individual had a flu shot in the previous year, 3) whether the individual binge drinks (5 or more drinks on one occasion) at least monthly, 4) whether the individual is a current smoker, 5) the number of occasions the individual exercises for at least 10 minutes per week, and 6) whether the individual had no doctor's visits in the previous year.

Table 8 shows the reduced-form effects of public 2-year access on 20 different health behaviors and outcomes in NHIS for 1) all individuals, 2) whites, and 3) whites age 50 and older. The first thing to note is that in spite of the fact that 2-year college access is strongly associated with years of schooling in the full NHIS sample, it has a statistically significant effect on only one of the 20 health outcomes we consider (monthly binge drinking, where the effect is positive and

[11] None of the individual coefficients from regressing health outcomes on 2-year college access are statistically significant among people of color (with the exception of current smoking, which is perversely positively related to access at the 10% level), which is consistent with the lack of an effect on schooling attainment for this group.

significant at the 10% level).[12] When we focus on whites alone (column 2), 2-year college access has a significantly positive effect on self-reported health (1% level), a significantly negative effect on the probability of stroke/heart attack (10% level), and a significantly negative effect on usual hours of sleep (5% level). This is a group for which the effect of 2-year college access on educational attainment is strong in both datasets and for which we find evidence of a positive effect of access on income in Census/ACS. Yet based on the measures we employ, there is only weak evidence of health effects, particularly given that a few significant results out of 20 outcomes may be due to statistical chance.

The third column of Table 8 focuses on whites age 50 and up. Because health problems may take years to develop for individuals who invest less in their health, we might expect to see the largest effects of college access on health outcomes among older adults. Indeed, we find that 2-year access has significant health-protecting effects (at the 10% level or better) on self-reported health, stroke/heart attack, diabetes/hypertension/heart disease, and mortality for this group. Even though the argument above does not imply that health *behaviors* are more likely to be affected by college access for older individuals, there is also a negative effect on current smoking for this group (significant at the 10% level). The coefficients imply that a one standard deviation increase in access raises the probability of excellent/very good health by about 0.7 percentage points, reduces the probabilities of stroke/heart attack by 0.5 percentage points and diabetes/hypertension/heart disease by 0.9 percentage points, and reduces the probability of death by 0.4 percentage points.

We next turn our attention to how any effects of 2-year college access on our health measures for (white) individuals vary by gender. Galama, Lleras-Muney, and van Kippersluis (2018) conclude that most studies that find effects of education on mortality and smoking find larger effects for men. This could be due to 1) typically used instruments having a larger effect on the education levels of men relative to women, or 2) a larger effect of a unit of education on health for men than women, perhaps because health-harming behaviors are more common among men (so that there is greater scope for education to reduce to such behaviors), physiological differences by gender, or something else.

Table 9 displays the effects of 2-year college access on our health measures comparing 1) all men to all women and 2) men age 50 and up to women age 50 and up. The first row shows that the effect of college access on years of schooling is substantially larger for men than women, especially among older cohorts (in which case the effect for women is not statistically significant at conventional levels). This would suggest that any effects of college access on health are likely to be larger for men, which is generally what we find. Among women, the only significant protective health effects (all at the 10% level) are on the likelihood of at least one doctor's visit (women 50 and older) and the likelihood of diabetes/hypertension/heart disease (both groups).

[12] Note that because the sample size differs markedly between specifications depending on the health measure in question (see Table 2), one might worry that the effect of 2-year college access on schooling is weak in some of these samples. However, this is not the case. Across the 20 different samples (each corresponding to a different health variable), regressing years of schooling on access produces coefficients that range from 2.9 to 4.1 and are always significant at the 5% level or better.

Among men, there are significant effects on self-reported health (both groups at the 1% level), current smoking (older men, 1% level), exercise (older men, 10% level), stroke/heart attack (both groups, 5% level), and deceased (older men, 1% level).[13]

A broad view of the results in Table 9 suggests that the group most affected by 2-year college access in terms of health outcomes are older (white) men. Along with the significant effects on outcomes mentioned above, there are meaningful though narrowly statistically insignificant health-enhancing effects on the likelihood of receiving a flu shot, activity limitation, and K6 mental distress. On the other hand, college access is also associated with a greater likelihood of binge drinking (significant at the 10% level) and lower back pain (5% level) among this group.[14]

A natural question is whether we can identify any intermediate variables that help explain why older (white) men experience gains in their health when they were exposed to more 2-year college access at young ages. We can look at a few such potential mechanisms in both Census/ACS and NHIS. These are contained in Table 10. The first two columns show the effect of public 2-year access on years of schooling specifically for white males age 50 and over in Census/ACS and NHIS (note that the coefficient in the latter case is the same as the one in Table 9). Though the coefficient is somewhat larger in NHIS, there is a strong relationship between 2-year colleges per capita and years of schooling in both datasets.

In terms of other variables, we first examine family income as in Tables 5 and 6 (but for older white males specifically). In the Census sample, the coefficient is positive and marginally statistically significant, indicating that a one standard deviation in college access leads to a 0.7 percent increase in family income. Table 6 indicated that income effects of increased college attainment due to greater 2-year access are generally larger at younger ages than 50, though if we had access to measures of *wealth* in our data, those might indicate increasing effects over the life cycle (unfortunately, our data do not contain wealth measures). We are not able to examine income in the NHIS data because it is a categorical variable for which categories change over the sample years, making it difficult to compare the variable over time.

The rest of Table 10 shows there is no strong evidence that 2-year college access affects the probability of employment, the number of children, or the probability of having health insurance

[13] It is of course impossible for us to separate age effects from cohort and year effects without further assumptions. We do not observe all cohorts at the same ages in our data, but we can try to rule out cohort effects (abstracting from year effects) by focusing only on early cohorts we observe at both younger and older ages. When limiting our analysis to cohorts who turned 17 between 1960 and 1975 (roughly 90% of those who are observed at age 50 or above at some point in our data) and compare health outcomes for those ages 40- 49 to those age 50 and above, we again find that it is only for the latter group that college access is significantly associated with smoking, exercise, stroke/heart attack, and mortality.

[14] The next logical step in our analysis of the health outcomes of older white men would be to use 2-year college access as an instrument for years of schooling in an IV regression of certain health outcomes on schooling. Unfortunately, when we test this instrument for weakness among this particular sample, we find the instrument is not sufficiently strong to make such estimates reliable.

among this older group of individuals. It bears noting that college access may affect employment/health insurance prior to age 50, and indeed Table 6 indicates that is the case for employment. Without longitudinal data, however, we cannot examine this directly for the cohorts of individuals who are sampled at age 50 and above in the years our analysis covers. The lack of a significant effect on the likelihood of health insurance is curious given that roughly 90% of this older sample are below age 65 (when Medicare eligibility begins) and more educated individuals are generally more likely to be insured (and have more generous coverage, which we do not observe) over the time period we study (Assaf et al., 2009).

On the other hand, there is some evidence across both datasets that college access affects the likelihood that white men ages 50 and up are married at the time they are surveyed (though the effect is larger and more precisely estimated in NHIS). It is revealing that effects of 2-year college access on the likelihood of being married for older white women are small and statistically insignificant across both datasets (results not shown). It is thus plausible that aside from increased income due to greater educational attainment, one mechanism that explains better health outcomes for these men is marital status. This is consistent with a protective effect of marriage on health found in the literature that is especially strong for men (see Rendall et al., 2011).

Conclusion

Leveraging changes in the accessibility of 2 and 4-year colleges from the 1960’s to 1990’s and across states, we find that public 2-year colleges per capita at age 17 has a significant effect on white individuals’ schooling and adult earnings. Puzzlingly, these effects are not present for blacks or other individuals of color, something that is consistent with findings from studies examining older interventions such as ours but not more recent ones. For example, over a similar time period, Currie and Moretti (2003) do not find significant effects of college openings on educational attainment for blacks, and Connolly (2021) finds that point estimates for 2-year college openings on schooling are positive for whites and blacks but only those for whites are precisely estimated; furthermore, positive effects on longevity are concentrated among whites. Reasons for this difference are not clear, especially as more recent interventions have been shown to have strong effects on people of color (Carrell and Sacerdote, 2017; Denning, 2017; Evans et al., 2018).

Our estimates imply substantial market returns to higher education for white individuals in our sample, which is consistent with many papers in the 2-year college literature (see, for example, Jepsen, Troske, and Coomes, 2014; Belfield and Bailey, 2017; Denning, 2017). In our data, educational and post-educational outcomes improved when more 2-year colleges were available despite some individuals possibly substituting 2-year schooling for 4-year schooling, which some suggest is detrimental to final schooling levels and other outcomes (Reynolds, 2012; Zimmerman, 2014; Goodman, Hurwitz, and Smith, 2015).

In contrast, we find little evidence that 2-year college access affects the health of all (white) individuals. This is in spite of the fact that this instrument affects schooling at the upper end of the distribution, where other studies have found the strongest correlation with measures of better health. Indeed, college access has a better chance of changing an individual's schooling "track," their peer group, and their eventual socioeconomic status than do often-used instruments such as compulsory schooling laws. To the extent that our instrument for schooling is endogenous, it would likely bias toward finding protective health effects given the robust positive effects we find on schooling, employment, and earnings.

Our story is more nuanced than claiming that schooling has no causal effect on health. First, 2-year college access is significantly correlated with an individual rating their health as excellent or very good. This could be the result of at least four things: 1) statistical chance—given that we are examining 20 health outcomes, there is a substantial likelihood of finding at least one erroneously significant coefficient; 2) health is truly affected by increased schooling via 2-year college access, but we lack the objective health measures in our data that would validate the increase in (subjective) self-reported health; 3) individuals who had better access to public 2-year colleges (and achieved higher schooling) *perceive* their health to be better, on average, than others, but there is no objective difference between the two groups; and 4) there are heterogeneous treatment effects by age and gender, and the effects for older men are strong enough to influence the coefficient on self-reported health but not the coefficients on objective health measures in the full sample.

Second, we find stronger evidence for effects of college access on health outcomes for older individuals (age 50 and up), particularly men. One explanation is that older men experience the largest effects of college availability on schooling attainment in the cohorts we analyze (with the coefficient being roughly twice as large for older men as it is for older women). These findings are also consistent with health effects of greater schooling not being manifest until older ages, when health problems in general become much more common. They are also consistent with findings from other parts of the health returns-to-schooling literature, with many papers finding stronger effects for men than for women (see Galama, Lleras-Muney, and van Kippersluis, 2018). For older (white) men, we find significant health-enhancing effects on self-reported health, smoking, exercise, incidence of stroke/heart attack, and mortality.

Our results underscore the argument made by Galama, Lleras-Muney, and van Kippersluis (2018) and Lleras-Muney (2022) that the health returns to education appear to be more context-dependent than the financial returns are. Though we find compelling evidence of financial gains accruing to those (white) individuals who experience greater 2-year college access at age 17, gains in health appear limited. This is true for both health outcomes and health behaviors, the latter of which we would expect to be more malleable with respect to schooling even at younger ages (the mean age in our NHIS sample is about 42). For older men, the increase in college availability is not only correlated with higher income but also an increase in the probability of being married (after age 50), which has been shown in other work to have protective health effects, particularly for men. Thus, one key to understanding when and how increasing access to formal education affects health may be in how family relationships respond to increases in schooling. This is a promising avenue for future research.

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Figure 1:

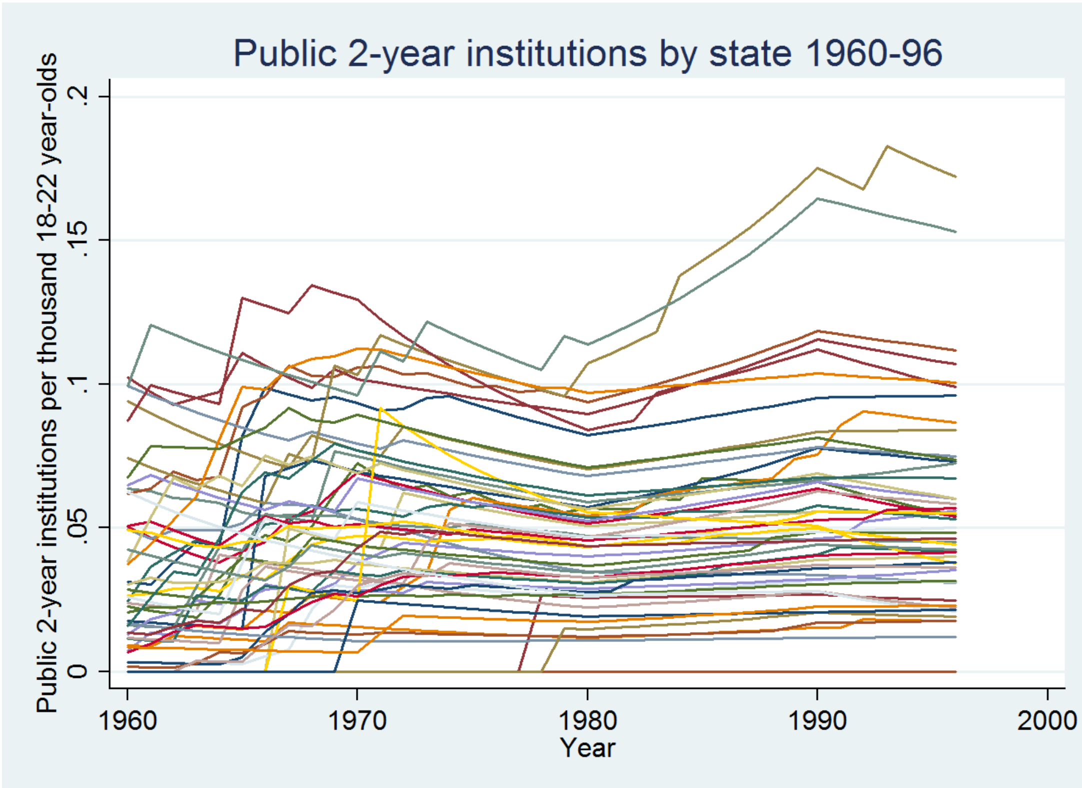

Figure 2:

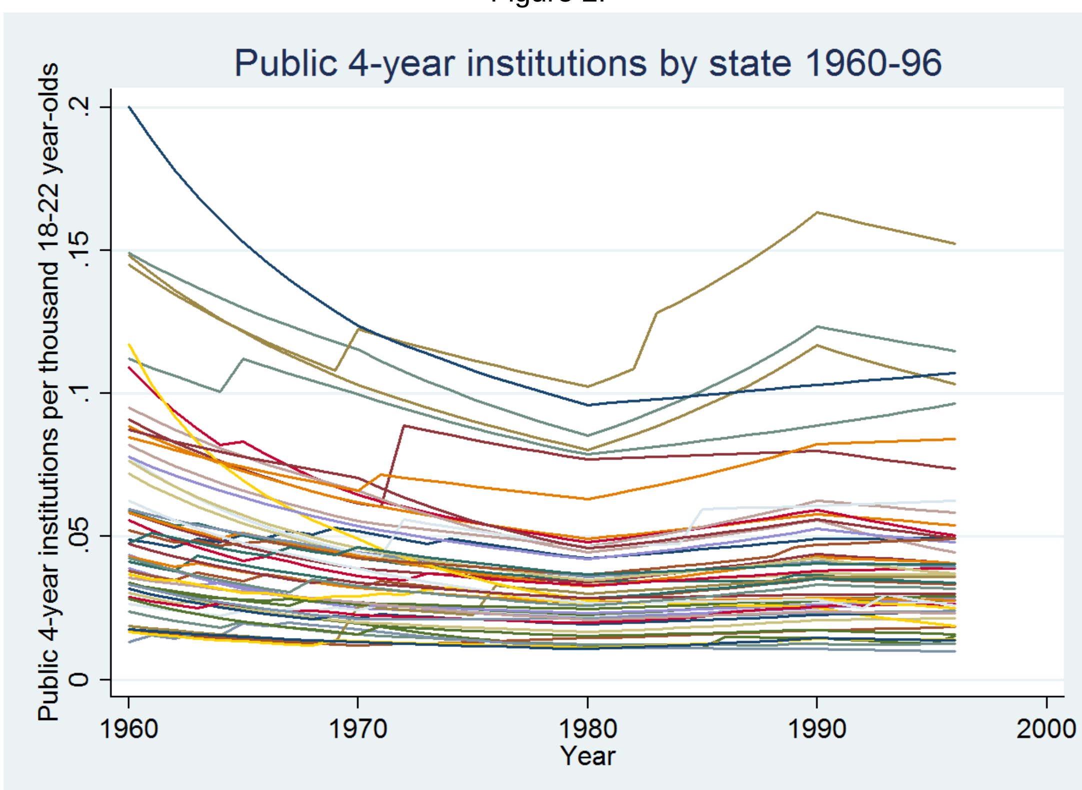

Figure 3:
Growth in public two-year colleges per capita, 1960-96

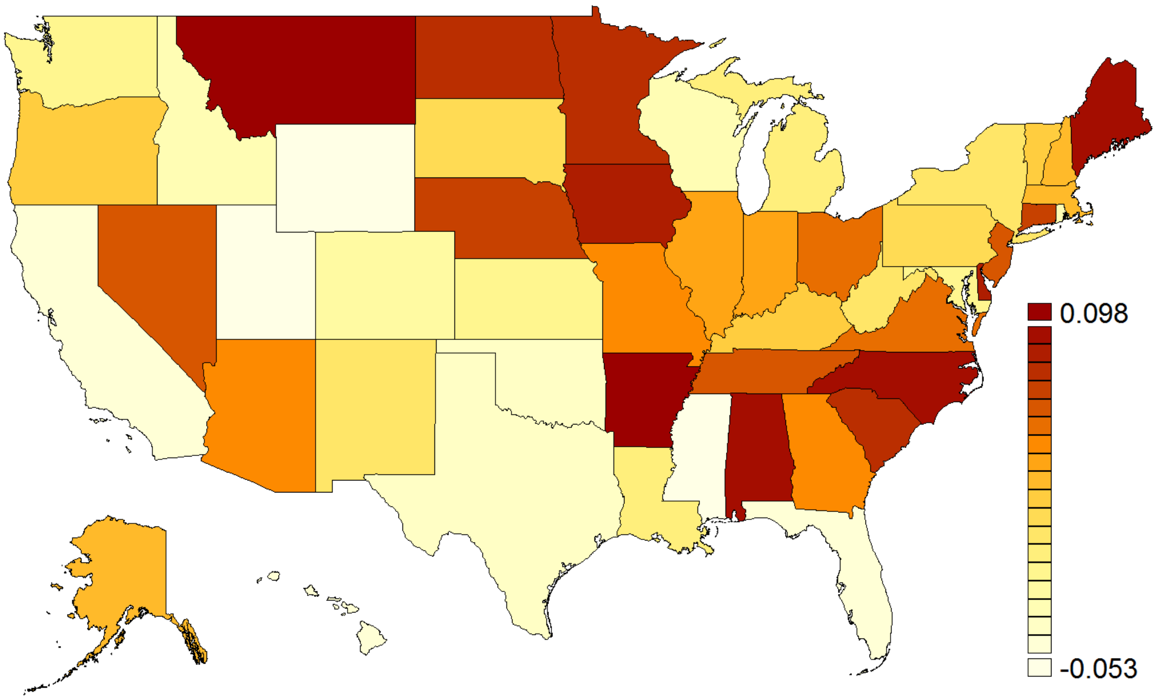

Figure 4:
Growth in public four-year colleges per capita, 1960-96

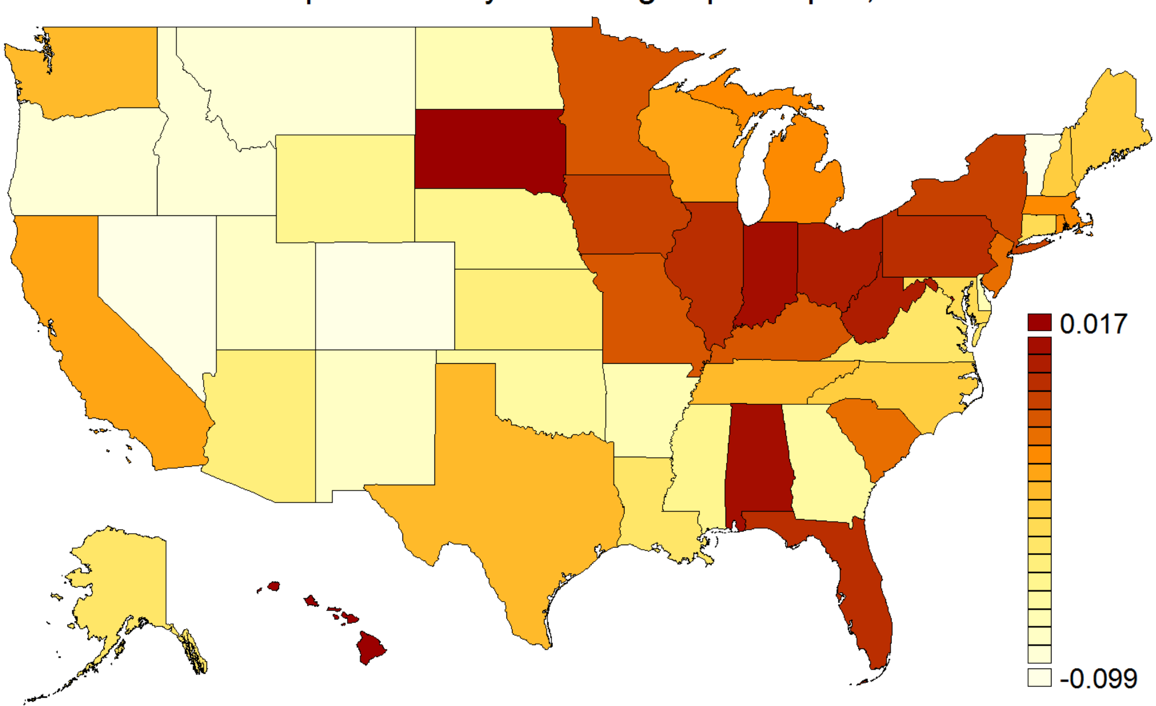

Figure 5:

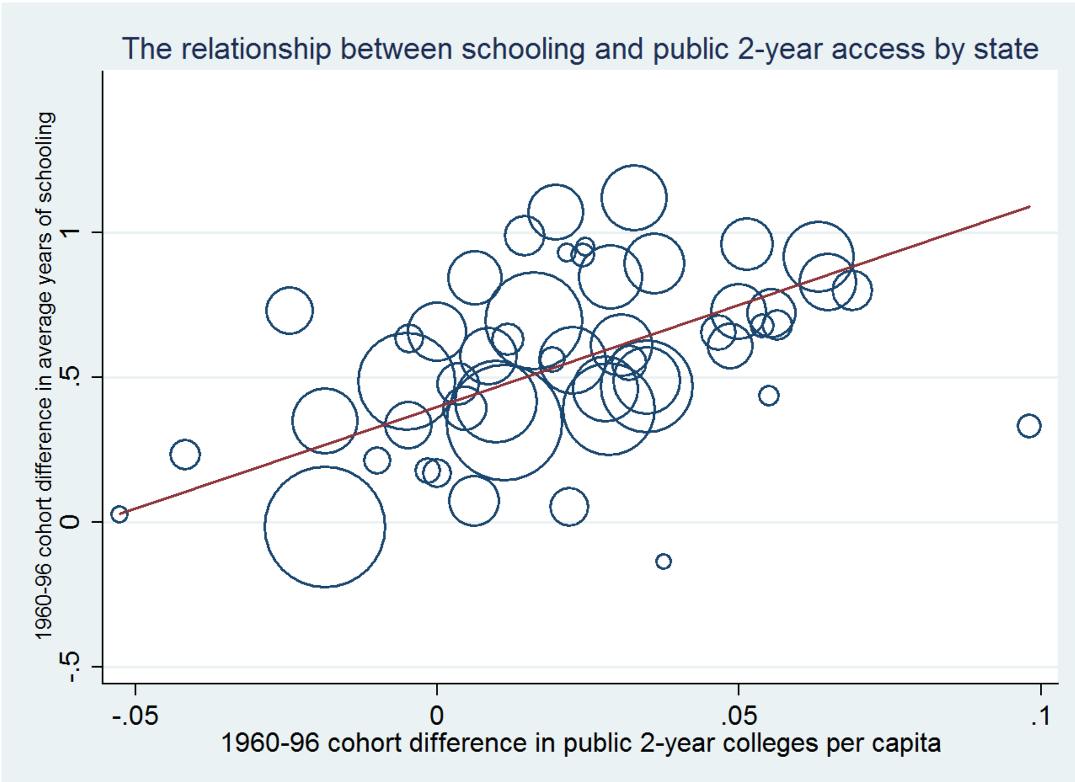

Figure 6:

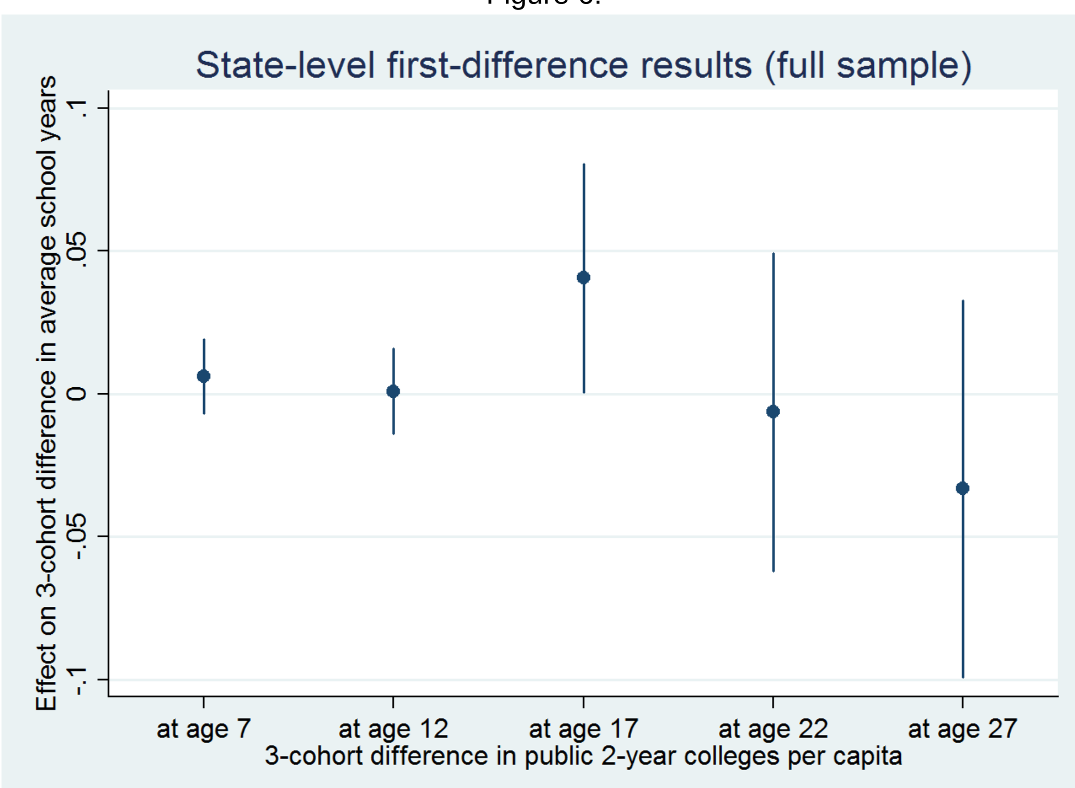

Figure 7:

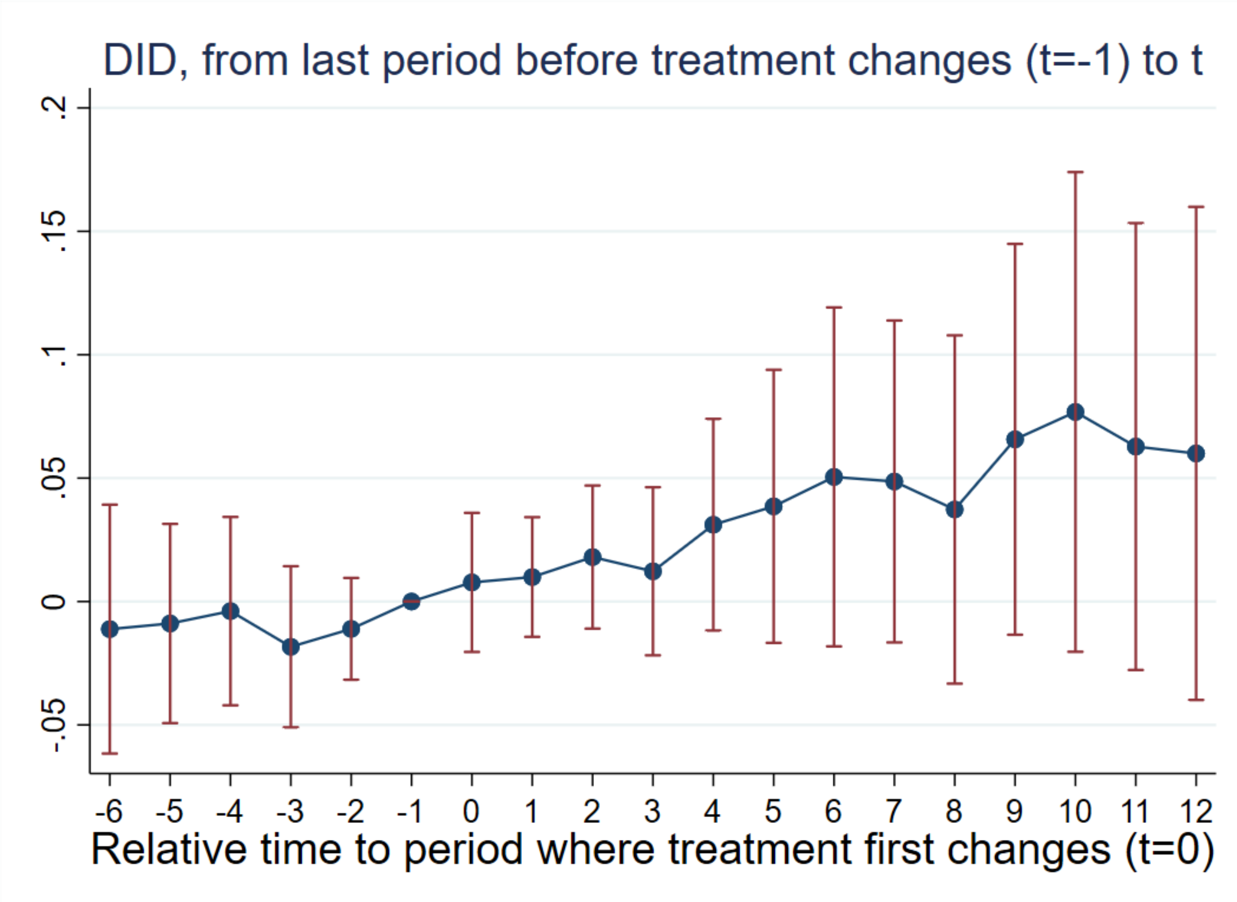
DID, from last period before treatment changes (t=-1) to t
-.05
0
.05
.1
.15
.2
-6 -5 -4 -3 -2 -1 0 1 2 3 4 5 6 7 8 9 10 11 12
Relative time to period where treatment first changes (t=0)

Table 1:

Summary statistics: 1980, 1990, and 2000 Census and 2001-15 ACS		
	mean	sd
Age	44.43	11.36
Years of schooling	13.42	0.38
Less than high school	0.08	0.04
High school diploma	0.38	0.05
Some college but no degree	0.25	0.04
College graduate	0.29	0.06
Log of personal income ($2012)	33,837	7,182
Log of family income ($2012)	63,773	11,611
Currenlty employed	0.72	0.12
Female	0.51	0.03
Person of color (non-white race)	0.18	0.11
Hispanic ethnicity	0.07	0.09
State public 4-year institutions per 1,000 18-22 year-olds, age 17	0.03	0.02
State public 2-year institutions per 1,000 18-22 year-olds, age 17	0.05	0.02
State private 4-year institutions per 1,000 18-22 year-olds, age 17	0.06	0.03
State private 2-year institutions per 1,000 18-22 year-olds, age 17	0.01	0.01
State per-capita income, age 17 ($1000's, 2012)	28.34	6.29
State per-pupil spending on K-12, age 17 ($1000's, 2012)	6.38	2.31
State unemployment rate, age 17	6.22	2.09

Sample is made up of state of birth-cohort-year averages for individuals age 22 and above who were 17 between 1960 and 1996 (N=31,959). Estimates are weighted by person sample weights within cells and state of birth populations of 18-22 year-olds in the year a cohort turns 17 across cells.

Table 2:

Individual summary statistics: 1984-2015 NHIS			
	N	mean	sd
Female	1,085,623	0.51	0.50
Age	1,085,623	41.57	11.32
Number of dependent children	1,085,623	1.01	1.19
Married	1,080,801	0.66	0.47
White race	1,083,467	0.85	0.36
Hispanic ethnicity	1,083,961	0.05	0.22
Years of schooling	1,075,761	13.67	2.44
Currently employed	1,081,296	0.77	0.42
No health insurance	945,926	0.14	0.34
Excellent or very good health	1,083,252	0.66	0.47
Usual hours sleep per day	208,418	7.04	1.33
Had flu shot in previous year	386,022	0.25	0.43
Obese (BMI>30)	698,890	0.20	0.40
More than 7 days in bed due to illness or injury, previous year	729,021	0.09	0.29
Binge drinks (5 or more drinks on one occasion) at least monthly	223,233	0.15	0.36
Current smoker	404,410	0.27	0.44
Number of occasions of at least 10 minutes of moderate exercise in last week	283,517	5.39	6.71
Activity limitation due to one or more chronic conditions	1,084,260	0.13	0.33
No visits to healthcare professional in previous year	433,069	0.28	0.45
Spent at least one night in hospital in previous year	1,085,431	0.08	0.27
Has had major depression, previous year	112,439	0.02	0.14
Kessler (K6) mental distress score	295,747	2.65	4.06
Has had lower-back pain in previous 3 months	300,398	0.31	0.46
Has had cancer	351,755	0.06	0.24
Has had a stroke or a heart attack	300,111	0.05	0.21
Has diabetes, hypertension, or coronary heart disease	300,015	0.29	0.46
Has emphysema or chronic bronchitis	300,290	0.06	0.24
Has other chronic condition (heart, kidney, liver, or ulcer)	299,929	0.16	0.36
Deceased	976,363	0.07	0.25

Sample is made up of individuals age 22 and above who were 17 between 1960 and 1996. Estimates are weighted by person sample weights. Sample sizes differ because some questions are asked in some years but not others as well as item non-response.

Table 3:

Effects of college access measures on educational outcomes, Census and ACS

	Less than HS	HS grad	Some college	College graduate	Years of schooling	Ln(personal income)	Ln(family income)	Employed
Public 2-year schools per 1,000 18-22 year-olds, age 17	-0.214**	-0.417***	0.214***	0.417***	2.667***	0.252	0.119	0.069
	(0.099)	(0.147)	(0.068)	(0.114)	(0.721)	(0.156)	(0.160)	(0.078)
Public 4-year schools per 1,000 18-22 year-olds, age 17	0.096	-0.652***	0.333**	0.223	1.009	-0.105	-0.319	-0.122
	(0.190)	(0.224)	(0.133)	(0.207)	(1.336)	(0.331)	(0.336)	(0.118)
Private 2-year schools per 1,000 18-22 year-olds, age 17	0.558*	0.093	-0.163	-0.488	-3.992**	0.021	-0.558	-0.374
	(0.294)	(0.463)	(0.222)	(0.292)	(1.787)	(0.501)	(0.510)	(0.280)
Private 4-year schools per 1,000 18-22 year-olds, age 17	0.070	-0.148	-0.051	0.128	0.342	-0.358*	-0.518**	-0.002
	(0.124)	(0.134)	(0.079)	(0.149)	(0.937)	(0.193)	(0.236)	(0.110)
R-square	0.789	0.657	0.650	0.778	0.841	0.593	0.664	0.706

*** p<0.01, ** p<0.05, * p<0.1. N=31,959. All models include state of birth, cohort, and year dummies; sex, race, and ethnicity dummies, state-level k-12 spending per pupil at age 17 (and its square), per capita income at age 17 (and its square), and unemployment rate at age 17 (and its square). Estmates are weighted by person sample weights within cells and state of birth populations of 18-22 year-olds in the year a cohort turns 17 across cells. Standard errors clustered at the state level.

Table 4:

Effects of college access measures on state-level characteristics						
	k-12 spending per pupil	Income per capita	Unemployment rate	Public 4-year schools	Private 4-year schools	Private 2-year schools
Public 2-year schools per	2.124	-1.981	1.263	0.046	0.028	0.032
1,000 18-22 year-olds, age 17	(4.267)	(8.677)	(5.374)	(0.036)	(0.073)	(0.033)
Public 4-year schools per	11.373	-12.077	16.715	---	0.397**	0.048
1,000 18-22 year-olds, age 17	(7.481)	(18.229)	(10.078)	---	(0.163)	(0.062)
Private 2-year schools per	31.194**	-40.939	-9.860	0.133	0.127	---
1,000 18-22 year-olds, age 17	(14.051)	(38.895)	(28.021)	(0.174)	(0.237)	---
Private 4-year schools per	-4.205	-18.015	-15.444	0.202***	---	0.023
1,000 18-22 year-olds, age 17	(5.842)	(16.286)	(10.751)	(0.071)	---	(0.045)
R-squared	0.968	0.981	0.780	0.962	0.973	0.922

*** p<0.01, ** p<0.05, * p<0.1. N=31,959. All models include state of birth, cohort, and year dummies; sex, race, and ethnicity dummies, state-level k-12 spending per pupil at age 17 and its square (except Column 1), per capita income at age 17 and its square (except Column 2), and unemployment rate at age 17 and its square (except Column 3). Estmates are weighted by person sample weights within cells and state of birth populations of 18-22 year-olds in the year a cohort turns 17 across cells. Standard errors clustered at the state level.

Table 5:

Effects of college access measures on educational outcomes by race/ethnicity, Census and ACS								
	Whites				People of color			
	Years of schooling	Ln(person al income)	Ln(family income)	Employed	Years of schooling	Ln(person al income)	Ln(family income)	Employed
Public 2-year schools per	3.535***	0.666***	0.524**	0.227**	0.649	-0.335	-0.570*	-0.134
1,000 18-22 year-olds, age 17	(0.768)	(0.231)	(0.207)	(0.110)	(1.059)	(0.326)	(0.319)	(0.131)
Observations	31,958	31,958	31,958	31,958	31,583	31,544	31,564	31,583
R-square	0.804	0.546	0.600	0.704	0.350	0.254	0.295	0.463

*** p<0.01, ** p<0.05, * p<0.1. All models include state of birth, cohort, and year dummies; sex dummies, state-level k-12 spending per pupil at age 17 (and its square), per capita income at age 17 (and its square), and unemployment rate at age 17 (and its square). Estmates are weighted by person sample weights within cells and state of birth populations of 18-22 year-olds in the year a cohort turns 17 across cells. Standard errors clustered at the state level.

Table 6:

Effects of 2-year college access on educational outcomes by age, Census and ACS				
	Years of schooling	Ln(personal income)	Ln(family income)	Employed
Age 30 and below (N=3,663)	2.403***	0.378	0.330	0.187
	(0.810)	(0.438)	(0.564)	(0.142)
Age 31-40 (N=8,244)	3.564***	0.841***	0.376	0.330**
	(1.055)	(0.277)	(0.284)	(0.131)
Age 41-50 (N=8,463)	2.857***	0.747**	0.561*	0.203**
	(0.775)	(0.321)	(0.312)	(0.077)
Age 51-60 N=7,712)	2.171***	0.392**	0.251	0.023
	(0.655)	(0.193)	(0.219)	(0.088)
Age 61 and above (N=3,876)	1.828***	-0.005	0.091	0.152
	(0.618)	(0.215)	(0.213)	(0.135)

*** p<0.01, ** p<0.05, * p<0.1. Sample is composed of whites. All models include state of birth, cohort, and year dummies; sex, race, and ethnicity dummies, state-level k-12 spending per pupil at age 17 (and its square), per capita income at age 17 (and its square), and unemployment rate at age 17 (and its square). Estmates are weighted by person sample weights within cells and state of birth populations of 18-22 year-olds in the year a cohort turns 17 across cells. Standard errors clustered at the state level.

Table 7:

Effects of college access measures on years of schooling, NHIS			
	All	Whites	People of color
Public 2-year schools per 1,000 18-22 year-olds, age 17	2.495***	2.953***	1.592
	(0.922)	(0.965)	(1.657)
Public 4-year schools per 1,000 18-22 year-olds, age 17	1.992	3.108	-7.123
	(2.158)	(2.093)	(5.116)
Private 2-year schools per 1,000 18-22 year-olds, age 17	-6.459	-7.384*	3.376
	(3.947)	(3.862)	(7.869)
Private 4-year schools per 1,000 18-22 year-olds, age 17	1.072	0.765	1.739
	(1.329)	(1.360)	(2.385)
Observations	1,067,117	875,206	191,911
R-square	0.051	0.041	0.056

*** p<0.01, ** p<0.05, * p<0.1. All models include state of birth, cohort, year, and sex dummies; quadratics of the following variables based on state of birth: k-12 spending per pupil at age 17, per capita income at age 17, unemployment rate at age 17; and quadratics of the following variables based on current state: current unemployment rate, cigarette tax, and beer tax. Estimates are weighted by person sample weights. Standard errors are clustered at the state of birth level.

Table 8:

Effects of 2-year college access on health outcomes, NHIS			
	All	Whites only	Whites>=50
Excellent or very good health	0.115	0.258***	0.373**
	(0.086)	(0.083)	(0.173)
Usual hours sleep per day	-0.601	-0.972**	-0.316
	(0.406)	(0.467)	(0.742)
Had flu shot in previous year	0.055	0.120	0.247
	(0.104)	(0.111)	(0.228)
Obese (BMI>30)	0.112	0.115	0.068
	(0.073)	(0.088)	(0.162)
More than 7 days in bed due to illness or injury,	0.038	0.019	0.069
previous year	(0.085)	(0.085)	(0.160)
Binge drinks (5 or more drinks on one occasion) at	0.283*	0.218	0.264
least monthly	(0.153)	(0.156)	(0.186)
Current smoker	0.159	0.012	-0.239*
	(0.105)	(0.133)	(0.135)
Number of occasions of at least 10 minutes of	2.545	2.340	2.399
moderate exercise per week	(1.898)	(2.307)	(3.201)
Activity limitation due to one or more chronic conditions	0.032	-0.025	-0.073
	(0.098)	(0.086)	(0.133)
No visits to doctor in previous year	0.129	0.130	-2.788
	(0.115)	(0.112)	(3.037)
Spent at least one night in hospital in previous year	-0.006	-0.008	0.100
	(0.051)	(0.052)	(0.089)
Has had major depression, previous year	-0.053	0.003	0.010
	(0.226)	(0.061)	(0.830)
Kessler (K6) mental distress score	0.637	0.204	-1.353
	(1.358)	(1.559)	(1.891)
Has had lower-back pain in previous 3 months	0.145	0.121	0.210
	(0.139)	(0.147)	(0.192)
Has had cancer	-0.013	-0.021	-0.103
	(0.091)	(0.100)	(0.158)
Has had a stroke or a heart attack	-0.083	-0.151*	-0.255**
	(0.072)	(0.086)	(0.109)
Has diabetes, hypertension, or coronary heart disease	-0.034	-0.142	-0.437*
	(0.135)	(0.154)	(0.248)
Has emphysema or chronic bronchitis	-0.021	-0.031	-0.132
	(0.068)	(0.079)	(0.115)
Has other chronic condition (heart, kidney, liver, or	-0.006	0.012	-0.129
ulcer)	(0.104)	(0.098)	(0.148)
Deceased	0.005	-0.008	-0.202**
	(0.057)	(0.053)	(0.087)

*** p<0.01, ** p<0.05, * p<0.1. All models include state of birth, cohort, year, and sex dummies; quadratics of the following variables based on state of birth: k-12 spending per pupil at age 17, per capita income at age 17, unemployment rate at age 17; and quadratics of the following variables based on current state: current unemployment rate, cigarette tax, and beer tax. Estimates are weighted by person sample weights. Standard errors are clustered at the state of birth level.

Table 9:

Effects of 2-year college access on health outcomes, NHIS				
	Male	Female	Male>=50	Female>=50
Years of schooling	3.967***	2.070**	3.820**	1.361
	(1.020)	(1.006)	(1.484)	(1.169)
Excellent or very good health	0.436***	0.088	0.647***	0.122
	(0.104)	(0.094)	(0.189)	(0.215)
Usual hours sleep per day	-1.441*	-0.590	-0.307	-0.317
	(0.778)	(0.639)	(1.155)	(1.045)
Had flu shot in previous year	0.037	0.192	0.371	0.143
	(0.131)	(0.183)	(0.250)	(0.333)
Obese (BMI>30)	0.118	0.112	0.315	-0.133
	(0.126)	(0.114)	(0.258)	(0.180)
More than 7 days in bed due to illness or injury, previous year	-0.053	0.093	0.045	0.092
	(0.086)	(0.115)	(0.141)	(0.229)
Binge drinks (5 or more drinks on one occasion) at least monthly	0.349	0.079	0.594*	-0.027
	(0.252)	(0.133)	(0.302)	(0.193)
Current smoker	-0.032	0.053	-0.798***	0.253
	(0.191)	(0.155)	(0.264)	(0.185)
Number of occasions of at least 10 minutes of moderate exercise per week	5.718	-0.341	9.554*	-3.634
	(3.793)	(2.284)	(5.104)	(3.140)
Activity limitation due to one or more chronic conditions	-0.120	0.065	-0.262	0.106
	(0.096)	(0.105)	(0.157)	(0.159)
No visits to doctor in previous year	0.118	0.122	-1.575	-4.069*
	(0.163)	(0.138)	(6.002)	(2.143)
Spent at least one night in hospital in previous year	0.002	-0.006	0.188	0.014
	(0.051)	(0.082)	(0.116)	(0.124)
Has had major depression, previous year	-0.000	0.026	-1.415	1.303
	(0.090)	(0.099)	(1.095)	(1.307)
Kessler (K6) mental distress score	-1.373	1.551	-3.457	0.353
	(1.865)	(1.715)	(2.657)	(2.414)
Has had lower-back pain in previous 3 months	0.304	-0.050	0.619**	-0.160
	(0.185)	(0.181)	(0.290)	(0.265)
Has had cancer	0.113	-0.127	0.084	-0.242
	(0.123)	(0.137)	(0.245)	(0.193)
Has had a stroke or a heart attack	-0.296**	-0.013	-0.411**	-0.095
	(0.143)	(0.098)	(0.166)	(0.162)
Has diabetes, hypertension, or coronary heart disease	0.089	-0.321*	-0.298	-0.532*
	(0.204)	(0.187)	(0.329)	(0.292)
Has emphysema or chronic bronchitis	0.018	-0.074	-0.032	-0.209
	(0.101)	(0.104)	(0.165)	(0.165)
Has other chronic condition (heart, kidney, liver, or ulcer)	0.078	-0.035	0.034	-0.239
	(0.141)	(0.138)	(0.254)	(0.275)
Deceased	-0.120	0.109*	-0.356***	-0.036
	(0.072)	(0.064)	(0.115)	(0.128)

*** p<0.01, ** p<0.05, * p<0.1. Sample is composed of whites. All models include state of birth, cohort, year, and sex dummies; quadratics of the following variables based on state of birth: k-12 spending per pupil at age 17, per capita income at age 17, unemployment rate at age 17; and quadratics of the following variables based on current state: current unemployment rate, cigarette tax, and beer tax. Estimates are weighted by person sample weights. Standard errors are clustered at the state of birth level.

Table 10:

Effects of college access measures on demographic and socioeconomic characteristics, NHIS

	Years of schooling		Log family income		Employed		Married		Number of children		No health insurance	
	Census	NHIS	Census	NHIS	Census	NHIS	Census	NHIS	Census	NHIS	Census	NHIS
Public 2-year schools per 1,000 18-22 year-olds, age 17	2.552***	3.820**	0.363*	N/A	0.137	0.004	0.110*	0.426***	0.206	-0.260	N/A	-0.088
	(0.692)	(1.484)	(0.211)		(0.103)	(0.223)	(0.061)	(0.152)	(0.188)	(0.342)		(0.139)

*** p<0.01, ** p<0.05, * p<0.1. Sample is composed of white males age 50 and over. See notes on other tables for Census/ACS and NHIS sample construction as well as covariates included in the regression models. Standard errors clustered at the state level.

Appendix Table 1:

Instrumental variable estimate of the return to schooling, Census and ACS

	Ln(income)
Years of schooling	0.086*
	(0.048)
Observations	31,959
F-stat on excluded instrument	12.45

*** p<0.01, ** p<0.05, * p<0.1. All models include state of birth, cohort, and year dummies; sex, race, and ethnicity dummies, state-level k-12 spending per pupil at age 17 (and its square), per capita income at age 17 (and its square), and unemployment rate at age 17 (and its square). Standard errors clustered at the state level. Years of schooling instrumented with state-level public 2-year colleges per capita. Estmates are weighted by person sample weights within cells and state of birth populations of 18-22 year-olds in the year a cohort turns 17 across cells. Standard errors clustered at the state level.

Appendix Table 2:

Effects of college access measures on educational outcomes by gender, Census and ACS								
	Female				Male			
	Years of schooling	Ln(personal income)	Ln(family income)	Employed	Years of schooling	Ln(personal income)	Ln(family income)	Employed
Public 2-year schools per 1,000 18-22 year-olds, age 17	3.274***	0.656***	0.422**	0.194*	3.915***	0.563**	0.528**	0.197**
	(0.775)	(0.242)	(0.197)	(0.115)	(0.774)	(0.223)	(0.201)	(0.097)
Observations	31,958	31,957	31,958	31,958	31,958	31,958	31,958	31,958
R-square	0.769	0.546	0.535	0.648	0.721	0.488	0.562	0.693

*** p<0.01, ** p<0.05, * p<0.1. Sample is composed of whites. All models include state of birth, cohort, and year dummies; sex dummies, state-level k-12 spending per pupil at age 17 (and its square), per capita income at age 17 (and its square), and unemployment rate at age 17 (and its square). Estmates are weighted by person sample weights within cells and state of birth populations of 18-22 year-olds in the year a cohort turns 17 across cells. Standard errors clustered at the state level.

Appendix Table 3:

Effects of 2-year college access on health outcomes, People of Color	
Excellent or very good health	-0.156
	(0.155)
Usual hours sleep per day	1.016
	(0.977)
Had flu shot in previous year	0.061
	(0.287)
Obese (BMI>30)	0.127
	(0.159)
More than 7 days in bed due to illness or injury, previous year	-0.041
	(0.160)
Binge drinks (5 or more drinks on one occasion) at least monthly	0.396
	(0.271)
Current smoker	0.352*
	(0.206)
Number of occasions of at least 10 minutes of moderate exercise per week	4.443
	(2.706)
Activity limitation due to one or more chronic conditions	0.040
	(0.157)
No visits to doctor in previous year	0.254
	(0.245)
Spent at least one night in hospital in previous year	-0.095
	(0.117)
Has had major depression, previous year	-0.030
	(0.129)
Kessler (K6) mental distress score	1.509
	(2.157)
Has had lower-back pain in previous 3 months	-0.053
	(0.226)
Has had cancer	0.121
	(0.132)
Has had a stroke or a heart attack	0.170
	(0.138)
Has diabetes, hypertension, or coronary heart disease	-0.021
	(0.221)
Has emphysema or chronic bronchitis	0.009
	(0.096)
Has other chronic condition (heart, kidney, liver, or ulcer)	-0.241
	(0.256)
Deceased	-0.142
	(0.134)

*** p<0.01, ** p<0.05, * p<0.1. All models include state of birth, cohort, year, and sex dummies; quadratics of the following variables based on state of birth: k-12 spending per pupil at age 17, per capita income at age 17, unemployment rate at age 17; and quadratics of the following variables based on current state: current unemployment rate, cigarette tax, and beer tax. Estimates are weighted by person sample weights. Standard errors are clustered at the state of birth level.

Appendix Table 4:

More detail on health outcomes, NHIS	
Variable	Details
Excellent or very good health	Derived from question regarding individual's self-assessed general health. Categorical answers. Equal to "1" if answer is "excellent" or "very good." Equal to "0" if answer is "good," "fair," or "poor."
Usual hours sleep per day	How many hours, on average, the respondent sleeps in a 24-hour period.
Had flu shot in previous year	Whether or not individual had a flu shot vaccine in the previous 12 months.
Obese (BMI>30)	Derived using individual's self-reported height and weight.
More than 7 days in bed due to illness or injury, previous year	Derived from question regarding number of days in bed for more than half the day, including while hospitalized. Categorical answers. Equal to "1" if answer is "8-30 days, "31-180 days," or "181-365 days." Equal to "0" if answer is "None" or "1-7 days."
Binge drinks (5 or more drinks on one occasion) at least monthly	Derived from a question about the number of days in the previous year that the respondent had 5 or more alcoholic drinks. Equal to "1" if this answer is 12 or more (and "0" otherwise).
Current smoker	Equal to "1" if individual indicates being a current smoker of any frequency (and "0" otherwise).
Number of occasions of at least 10 minutes of moderate exercise per week	Number of occasions per week of at least 10 minutes of "moderate" physical activity (light sweating or slight to moderate increase in heart rate), "vigorous" physical activity (heavy sweating or large increase in heart rate), or muscle-strengthening activities.
Activity limitation due to one or more chronic conditions	Equal to "1" if person is limited in any way in their activites due to chronic condition(s) (and "0" otherwise).
No visits to doctor in previous year	Derived from question regarding the number of visits to a medical doctor or assistant in previous year (excluding inpatient hospital visits).
Spent at least one night in hospital in previous year	Derived from a question regarding the number of overnight hospital visits in previous year (emergency room visits are excluded).
Has had major depression, previous year	Equal to "1" if person indicated having "major" depression (a depressed mood and loss of interest in almost all activities for at least two weeks) or "manic" depression (bipolar disorder) in the past year (and "0" otherwise).
Kessler (K6) mental distress score	Scale ranging from 0-24 depending on frequency (over the past 30 days) of feeling unmotivated, hopeless, nervous, restless, sad, and worthless.
Has had lower-back pain in previous 3 months	Whether or not individual had low back pain last at least one day and occuring more than 3 times in the previous 3 months.
Has had cancer	Equal to "1" if person indicated having ever been diagnosed with cancer by a doctor or other health professional (and "0" otherwise).
Has had a stroke or a heart attack	Equal to "1" if person indicated having ever been diagnosed with angina pectoris, heart attack (myocardial infarction), or stroke (and "0" otherwise).
Has diabetes, hypertension, or coronary heart disease	Equal to "1" if person indicated having ever been diagnosed with coronary heart disease, diabetes (other than during pregnancy), or hypertension (high blood pressure) (and "0" otherwise).
Has emphysema or chronic bronchitis	Equal to "1" if person indicated having been diagnosed with chronic bronchitis in the past 12 months or having ever been diagnosed with emphysema (and "0" otherwise).
Has other chronic condition (heart, kidney, liver, or ulcer)	Equal to "1" if person indicated having ever been diagnosed with a heart condition (other than coronary heart disease, myocardial infarction, or angina pectoris), an ulcer, weak or failing kidneys in the past year, or chronic liver condition in the past year (and "0" otherwise).
Deceased	Based on probabilistic matches with National Death Index records for respondents who provided sufficient data for linking.